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The following communications were read:—

I. On the Mass of *Jupiter*. By Professor Airy.

"Next to the elements of the planetary orbits, the most important numerical value for the explanation and prediction of the phenomena of the solar system, is the mass of Jupiter. The great influence of this planet on the motions of *Saturn* was loosely pointed out by Newton; in *Halley's Tables* it was remarked that two successive periods of *Saturn* were found, from observation, to differ thirteen days; the small planets discovered in the present century are even more affected (in angular perturbation) by *Jupiter's* attraction; and, lastly, the motions of the comets of short period are changed in a still greater degree by his enormous perturbing force. The admission or rejection of the theory of the resistance of Encke's comet depended at one time upon the use of one or the other of the two values of *Jupiter's* mass, which were sanctioned by different authorities."

"With a full knowledge of this importance, it seems remarkable that no attempts should lately have been made to ascertain the correct value of *Jupiter's* mass, from the readiest of all means, the elongation of his satellites. It is true, that the value adopted by Laplace, $\frac{1}{1067\cdot09}$ of the sun's mass, (*Méc. Cél.* vol. iii. p. 72), was inferred from observations of the fourth satellite; but they were the observations which were made by Pound, and of which no account (so far as I know) remains, except the mere statement of numbers in the *Principia*. All the modern methods are comparatively indirect. Bouvard's was obtained from the perturbations of *Saturn*, as ascertained from modern observation (Introduction to his *Tables*), in the course of which (it may be remarked) the great inequality of *Saturn* depending on *Jupiter's* attraction is not particularly sensible. His numerical expression $\frac{1}{1070\cdot5}$ differed little from Laplace's. Nicolai, from the perturbations of *Juno*, found a value $\frac{1}{1053\cdot924}$, differing from Laplace's by $\frac{1}{80}$th part of the whole (*Berl. Jahrbuch*, 1826); and this discordance seems to have been the principal cause

of the question so much agitated among German astronomers; — Whether the absolute attraction of *Jupiter* on all bodies is the same? Encke (*Berlin Memoirs*, 1826) obtained from the perturbations of *Vesta* a value $\frac{1}{1050 \cdot 117}$, differing little from that of Nicolai; and Gauss, it is understood (for I am not aware that any account is published), has deduced the same, or nearly the same, from the perturbations of *Pallas*. Finally, the equations of condition from which the coefficient in the resistance of Encke's comet was determined (*Astronomische Nachrichten*, No. 210), served at the same time to correct the mass of *Jupiter;* and the value thus obtained, $\frac{1}{1054 \cdot 4}$, agrees very closely with that deduced from the perturbations of the three small planets."

"The best of the modern estimations thus conspiring to shew the inaccuracy of that which had been obtained from the observation of the satellites, it became a matter of great interest to inquire whether the difference proceeded from the defects of Pound's observations and theory, or from any physical difference in the action of *Jupiter* upon these two sets of bodies. Before the erection of the Cambridge equatorial (in the summer of 1832), I had determined, as soon as it should be fit for use, to make a series of observations on the fourth satellite for this purpose. My absence from Cambridge prevented me from making any observations till the month of September. In the course of that month, and the two following, thirteen sets of measures were obtained. The first of these was never calculated; I shall give good reasons for also rejecting the second and third (though, if retained, their effect on the result will be insensible); the remaining ten I believe to be as good as the nature of the observation admits of. The result as to the mass of *Jupiter* is highly satisfactory. The value thus obtained from the elongations of the fourth satellite agrees almost precisely with that obtained by Encke from the perturbations of *Vesta*, and cannot in any way be reconciled with that which was adopted by Laplace, and in some degree confirmed by the investigations of Bouvard."

"I shall endeavour to give means of estimating the accuracy of my result: in the mean time I may remark, that I consider it at least as accurate as those above quoted, for the following reason. Encke, after giving his own value, states that Nicolai's value would satisfy his equations nearly as well, and the numbers which I shall cite will prove the correctness of this remark. Now, for my equations Encke's value (with which mine may be considered coincident) is distinctly better than Nicolai's. A difference, therefore, which is not sensible in Encke's investigations, does appear to be sensible in mine."

"The detail of the observations will be found in the fifth volume of the *Cambridge Observations* for 1832."

The observations employed by Professor Airy for this investigation were differences of right ascension between the fourth satellite and the planet about the times of greatest elongation.

The distance of the fourth satellite from the body of *Jupiter* was in general great enough to admit of the transit observation being made at three wires; but sometimes only two were used. This operation was repeated twelve times; the instrument was then turned round 180°, and the same number of transits observed in the reversed position; and as the difference in declination between the two bodies was considerable, the declination circle being unclamped, was moved by hand, so as to use the same part of the wires in each case. It is obvious, that even if the axis of the declination circle be not truly perpendicular to the polar axis, the *mean* of results in reversed positions will be free from instrumental error arising from such a cause. In other respects the equatorial was sufficiently well adjusted; and the observations made on two days, when the former precautions were neglected, are omitted.

After explaining the mode of observation, and the reductions required for obtaining the true difference of right ascension, Professor Airy shews how the position and length of the radius vector of the satellite are found from the data of the *Mécanique Céleste;* "the length requiring, for its complete numerical expression, to be multiplied by the mean distance of the satellite from *Jupiter* (which at present is undetermined); from these the position and length of the projection of the radius vector on the earth's equator are found."

"The *Berliner Jahrbuch* gives the right ascension, declination, and log. distance of *Jupiter* from the earth. From these the position and length (expressed in terms of the earth's mean distance from the sun) of the projection on the earth's equator of the line drawn from the earth to *Jupiter* may be found. Knowing this, together with the difference of right ascension of *Jupiter* and the satellite, as observed at the earth, and the terrestrial right ascension of the satellite as seen from *Jupiter*, the length of the projection of the radius vector on the earth's equator is found, expressed by a multiple of the earth's mean distance from the sun.

"Comparing this with the former expression (which was a multiple of the satellite's mean distance from *Jupiter*), the mean distance of the satellite is immediately found, and thence the mass of *Jupiter*."

"I had at first no intention of correcting, or even examining the numbers of the *Mécanique Céleste;* but an examination simply for the verification of the typographical accuracy, led me to doubt the correctness of several of the numbers in page 94 of the fourth volume, and of those which depend on them. On further examination, I found that the variable terms, both of the radius vector and longitude, as far as I examined, were nearly correct; but that in the constant parts of the radius vector some terms were wrong in sign, one erroneous in magnitude, and one omitted. I corrected these, and then proceeded in the construction of my tables."

"In the first instance, however, to which I applied the calculations, I found, to my great astonishment, that, at the time when I had observed the satellite at extreme elongation, the tables repre-

sented it as between the earth and *Jupiter*. I satisfied myself that this was owing to no error in my own calculations, and I then compared Laplace's numbers with those of Halley and Delambre. From the tables of the former I could only infer that there was some very strange error in Laplace's epoch for 1750; from the mean conjunctions of the latter I could calculate its quantity. It will scarcely be believed, that the epoch of mean longitude in page 139 of the fourth volume of the *Mécanique Céleste* is erroneous to the amount of nearly one-third of the circumference, and that this is not a typographical error, as several numbers are combined with it by addition and subtraction in the pages following it, and all correctly. I confess that my faith in the *numerical* results of the *Mécanique Céleste* has been very much weakened by these discoveries."

"At the same time, I am far from imputing any negligence to the author of that great work; for I know too well the difficulty of insuring correction in long calculations from complex formulæ: and in some of the fundamental numbers of the present computations I was saved from error only by Mr. Lubbock's re-calculations."

A list of the errata in the *Mécanique Céleste* is appended to the memoir. The following are the individual results:—

1832.	Log. mass of ♃	Pos. 4th satellite.
Oct. 5	6·9811676	Following.
6	6·9808121	Following.
13	6·9764252	Preceding.
21	6·9814433	Following.
22	6·9828392	Following.
Nov. 1	6·9796958	Preceding.
10	6·9809291	Following.
16	6·9749036	Preceding.
17	6·9766625	Preceding.
25	6·9830216	Following.

"We see immediately that every determination from observations when the satellite *followed*, is greater than every determination when the satellite *preceded*. There are two ways of explaining this. One is by an error in the excentricity; for the satellite at its greatest elongations was very nearly at its apsides. But I do not ascribe the difference to this cause, but to an involuntary difference in the mode of observing the transits of a planet's limbs and a satellite's centre. I believe, in fact, that the difference of right ascension found from transits would not be the same as the difference determined by a double-image micrometer, or any similar contrivance. It is proper to state clearly, that before the commencement of the observations, I had distinctly contemplated the possibility of such a

difference, and had in consequence determined to observe the satellite, if possible, on both sides of the planet. It is plain that the effect of such an error is to alter the difference of right ascension in one direction by a constant quantity when the satellite follows, and to alter it by the same quantity in the opposite direction when the satellite precedes: the effect of an error in excentricity would be very nearly similar."

"Suppose, now, this constant error to be c, and the variable errors of the observations, beginning with October 5, to be e_1, e_2, &c. (all in multiples of $0^s{\cdot}001$ of time). When the satellite *follows*, as in October 5, we must suppose the observed right ascension to be too great by $(e_1 + c) \times 0^s{\cdot}001$; and when it *precedes*, as on October 13, we must suppose the observed right ascension to be too great by $(e_3 - c) \times 0^s{\cdot}001$. As the mass (at any given time) is nearly proportional to the cube of the difference of right ascension, we may conceive that on October 5 the log. mass is determined by adding some constant C to 3 log. (36·440). Thus we have

$$C + 3\log.(36{\cdot}440) = 6{\cdot}9811676.$$

But if x be the true log. mass,

$$C + 3\log.(36{\cdot}440 - \overline{e_1 + c} \times 0{\cdot}001) = x.$$

The difference gives

$$3\log.\left(1 - \frac{\overline{e_1 + c} \times 0{\cdot}001}{36{\cdot}440}\right) = x - 6{\cdot}9811676.$$

"It is easily seen that

$$3\log.\left(1 - \frac{\overline{e_1 + c} \times 0{\cdot}001}{36{\cdot}440}\right) = \frac{-1}{767{\cdot}5 \times 36{\cdot}440} \times \overline{e^1 + c} \text{ nearly,}$$

(the number $767{\cdot}5 = \frac{1000}{3 \times \text{modulus}}$ being constant); hence we have

$$e_1 = -c + 767{\cdot}5 \times 36{\cdot}440 \times (6{\cdot}9811676 - x)$$

"Applying this process to the last ten observations, and putting $x = 6{\cdot}97 + y$, we get the following table of errors:—

$$\begin{aligned}
e_1 &= -c + 28000 \times (0{\cdot}0111676 - y)\\
e_2 &= -c + 30200 \times (0{\cdot}0108121 - y)\\
e_3 &= +c + 26000 \times (0{\cdot}0064252 - y)\\
e_4 &= -c + 23600 \times (0{\cdot}0114433 - y)\\
e_5 &= -c + 28300 \times (0{\cdot}0128392 - y)\\
e_6 &= +c + 26200 \times (0{\cdot}0096958 - y)\\
e_7 &= -c + 22000 \times (0{\cdot}0109291 - y)\\
e_8 &= +c + 26000 \times (0{\cdot}0049036 - y)\\
e_9 &= +c + 26600 \times (0{\cdot}0066625 - y)\\
e_{10} &= -c + 26500 \times (0{\cdot}0130216 - y)
\end{aligned}$$

"The difference in the accuracy of the different observations I conceive to be very small; for though some comprehended a

greater number of wires than others, the former were in general rather more hurried than the latter. Considering all equal in accuracy, and taking the sum of the squares of the errors, we find that it may be put in this form,

$$6986140000\left\{x + 0{\cdot}00000770096 \times c - 6{\cdot}9798374\right\}^2$$
$$+ 9{\cdot}58569\left\{c - 62{\cdot}9112\right\}^2$$
$$+ 12179{\cdot}1$$

" To make this a minimum with regard to c, it is plain that we must put $c = 62{\cdot}9112$; or the constant error in the difference of transits $= 0^s{\cdot}063$. The expression for the sum of the squares of errors then becomes

$$6986140000\left\{x - 6{\cdot}9793529\right\}^2 + 12179{\cdot}1$$

The value of x which makes this least is $6{\cdot}9793529$; and this is therefore the value which I adopt, subject to some very small corrections, to be mentioned hereafter. The number corresponding to this logarithm is $\frac{1}{1048{\cdot}69}$.

" To shew the reliance which can be placed upon this determination, I shall exhibit the amount of the sum of squares produced by the substitution of this and of other values of the log. mass."

" The value above gives 12179·1, whence the mean value of the square of an error is 1217·91; or the mean error of a difference of right ascension (independent of the constant error) $0^s{\cdot}035$."

" Encke's value of the log. mass is 6·9787626, which gives for the sum of squares 14613·0; or the mean error of a difference of right ascension $0^s{\cdot}038$."

" Nicolai's value is 6·9771907, which makes the sum of squares 44839·3; or the mean error of a difference of right ascension $0^s{\cdot}067$."

" Laplace's value is 6·9717989, which makes the sum of squares 410828·5; or the mean error of a difference of right ascension $0^s{\cdot}203$."

" It is here evident that Encke's value satisfies the observations nearly as well as mine, but that Nicolai's value by no means satisfies them so well; the sum of the squares of errors being more than three times as great as that given by Encke's, and nearly four times as great as that given by mine. Laplace's value is clearly incompatible with my observations."

" The preference of Encke's value to Nicolai's is much more distinctly marked here than in Encke's original investigations; the sum of the squares of the errors in the observations of *Vesta* was 59·65 with Encke's mass, and only 65·68 with Nicolai's mass. As far, therefore, as those observations went, Encke was justified in saying that Nicolai's mass satisfied them very nearly as well as his own, and that there was no sufficient reason for adopting his value in preference to Nicolai's. Here, however, the case is quite dif-

ferent; and we may assert, without hesitation, that Nicolai's mass cannot now be admitted, but that Encke's mass is nearly as well supported as mine by these observations."

"If the assumption of a constant error (changing its sign according as the satellite preceded or followed) were rejected, the expression for the sum of squares would be

$$6986140000\left\{x - 6{\cdot}9798374\right\}^2 + 50117{\cdot}5$$

The alteration in the value of the mass which makes the sum of squares a minimum, is very trifling. The values of the sum of squares and the mean error are now,

With my mass above	51757·1	and $0^s{\cdot}072$
With Encke's mass	58187·1	and $0^s{\cdot}076$
With Nicolai's mass	99055·4	and $0^s{\cdot}100$
With Laplace's mass	501544·5	and $0^s{\cdot}224$

"I have given these only to shew that the decided preference of Encke's mass to Nicolai's depends very little on the assumption of a constant error. It is, however, merely a matter of curiosity; for no one, I believe, will deny the certainty of the constant error in these observations. I may add, that as I have been accustomed to consider the error of a transit over seven wires as not greater than $0^s{\cdot}06$, when made under favourable circumstances, I may safely estimate the difference of right ascension, determined by passages over twenty-four or thirty-six wires (all which were chosen observations, unsatisfactory observations having been rejected at once), as perhaps liable to a mean error of $0^s{\cdot}035$, but certainly not liable to one of $0^s{\cdot}067$ or $0^s{\cdot}072$."

After applying two small corrections, one for the clock rate during the observations, and the other for obtaining the whole mass of the Jovial system, Professor Airy concludes, that we have for the log. mass of the Jovial system 6·9793486, and for the mass $\frac{1}{1048{\cdot}70}$."

"In conclusion I may remark, that I do not think this value liable to any serious error, except from a possible error in the inclination of the orbit of the fourth satellite. The effect of the greatest error of the instrument in azimuth (0′·5) that any observations have shewn, acting at the greatest disadvantage, would not alter the denomination of the fraction by 0·2. But the observations used here have been made when *Jupiter* was in the most unfavourable of all positions, both from the effect of such errors, and from the smallness of the difference of right ascension, which is the immediate subject of the measures. The oppositions of 1834-5 and 1836 will be very much more favourable."

"I am far from regarding the question as definitively settled by these measures; and I offer these results to the Society as only the first fruits of my investigations. Should the regular business of the Observatory, and my other employments, leave me sufficient

leisure, I hope to add considerably to their number. The necessary tables and forms of calculations are prepared; some theoretical and numerical difficulties have been surmounted; and the labour of calculating future observations will therefore be very much less than that of calculating the first."

II. On a Method of determining the Longitude with considerable accuracy by means of Lunar Eclipses. By Captain Henry Kater, F.R.S.

The author remarks that eclipses of the moon have hitherto been of but little use in the determination of terrestrial longitudes, on account of the uncertainty attending the observation of their commencement and termination. And although it has been proposed to observe the times when the earth's shadow touches certain spots upon the moon, yet it is evident that for the purpose of deducing the longitude from such observations, not only two observers are necessary, but they must also previously agree upon the particular spots to be observed. This mode, therefore, promises but little practical utility.

The method which the author here details enables the observer to deduce, with considerable accuracy, the time of the middle of the eclipse. It consists in taking numerous measures of the enlightened segment with a micrometer, near the commencement and end of the eclipse, noting the corresponding times. It is evident that by taking the mean of the times corresponding to any two equal segments near the beginning and end of the eclipse, the time of the middle will be obtained as accurately, if not more so, than it would have been if deduced from observations of the beginning and end of the eclipse made in the usual manner. And by taking the mean of many similar deductions of the middle of the eclipse, a result will be obtained approximating very nearly to the truth.

Captain Kater says, "I was not aware until lately that I have been in some measure anticipated by a suggestion of this method by M. La Lande, who says, in his *Astronomie*, 'On observe aussi les phases, ou les segmens éclairés, par le moyen du micromètre, pour déduire le milieu par deux phases égales.' I cannot, however, find that his suggestion was ever put in practice."

"The observations given in this paper were made merely with a view to determine the amount of the greatest obscuration, and consequently I did not begin my measurements until long after the commencement of the eclipse, and they were discontinued some time before its termination. In addition to which, they were not made with all the care that might have been bestowed upon them. It will therefore be seen, that the example I am about to give is nearly as unfavourable as can well be imagined. The accuracy of the result, under such circumstances, is the best proof of what may be expected from observations made with an express view to the object in question."

The author then states the result of forty-five micrometrical

measurements made by him at Union Place, during the lunar eclipse of August 2d, 1832: amongst which he selects several made after the middle of the eclipse, of nearly equal magnitude with others made prior thereto. Combining such similar measures, he thence deduces, from the mean of all the combinations, the middle of the eclipse, as shewn by the clock used in the observations.

By taking the difference between the computed mean time of the middle of the eclipse at Greenwich, and the observed mean time of the middle of the eclipse at Union Place, he obtains the longitude of Union Place from Greenwich as follows:

Longitude by each pair of measurements.	Error.	Number of obs.	Longitude by the mean.	Error.
m s	m s		s	s
W. 0 9,26	−0 26,94	1	W. 9,26	−26,94
E. 0 6,74	−0 42,94	2	1,26	−34,94
W. 1 52,26	+1 16,06	3	38,26	+ 2,06
1 9,26	+0 33,06	4	46,01	+ 9,81
0 25,26	−0 10,94	5	41,86	+ 5,66
0 4,26	−0 31,94	6	35,60	− 0,60
0 24,26	−0 11,94	7	33,98	− 2,22
0 53,26	+0 17,06	8	36,39	+ 0,19
1 58,26	+0 22,06	9	45,49	+ 9,29
1 1,26	+0 25,06	10	47,06	+10,86
1 18,26	+0 42,06	11	49,90	+13,70
1 2,26	+0 26,06	12	50,93	+14,73

"In the above table, the first column contains the longitude deduced from each pair of measurements; the second column, the error or difference of each result from the true longitude; the third column, the number of separate determinations of the time of the middle of the eclipse, the mean of which has been taken in determining the longitude, and which resulting longitude is given in the fourth column; the fifth column contains the error or difference of each mean from the true longitude."

"On considering the fifth column, containing the errors of the longitude deduced from the mean of several observations, it may be seen that when the mean of more than five results is taken, the error of the deduced longitude is very small; and it does not appear necessary to take the mean of more than seven or eight results, to obtain a near approximation to the truth. In the present instance, the last four or five means include observations made too near the middle of the eclipse to be relied upon, for it must be obvious that those made near the commencement and termination will give by far the most accurate results."

"It seems, then, that if we take the mean of the first six, the first seven, and the first eight results, the respective differences of

the deduced longitude from the truth would be $-0^s,6$, $-2^s,2$, and $+0^s,19$; and it is very probable that had the observations been made near the beginning and end of the eclipse, instead of so near the middle, the error of the resulting longitude would have been confined within very narrow limits."

"It is unnecessary to enlarge on the advantages which this method presents. It may be readily practised at sea by using a sextant: for this suggestion I am indebted to Captain Beaufort. It frequently happens that neither the beginning nor end of an eclipse can be observed, yet, as in the present instance, the time of the middle of the eclipse may, notwithstanding, be determined. It can rarely happen that only one pair of measurements of the luminous segments can be taken, yet even in that case it is probable that the longitude would be obtained, as may be seen by the above table, much within one minute of the truth, which is far within the limits of the errors assigned by different writers to observations made in the usual manner. But when the mean of a number of deductions of the middle time of the eclipse is taken, it should seem that it would be difficult to limit the degree of accuracy which it might be possible to attain."

As an eclipse of the moon will take place on the 1st of July, observers will have an opportunity of trying the efficacy of this method.

III. Planetary Observations made at the Observatory of Wilna. By M. Slavinski.

These observations, which are lithographed, consist of right ascensions and declinations of *Uranus* in 1829, and of *Mars*, *Jupiter*, *Uranus*, and *Vesta*, in 1830. The stars of comparison were all reduced by the Astronomical Society's Catalogue. The maximum, minimum, and mean heights of the barometer and thermometer for every month of the two years during which the observations were made, are annexed.

IV. A letter from the Rev. W. R. Dawes to Mr. Dollond, on an improvement in the Micrometer by the latter. Communicated by Mr. Dollond.

Mr. Dawes expresses a highly favourable opinion of Mr. Dollond's improvement, from the results of his own observations; but as the particulars of that improvement have not yet been communicated to the Society, the details of this paper are deferred till further information is received on the subject.

V. Observations, &c. made at Padua, from the year 1829 to 1832, inclusive. By M. Santini.

These observations comprise, 1st, Occultations of *Aldebaran* and other stars; 2d, the transit of *Mercury*, occultation of *Saturn*, and eclipse of the sun of 1832; 3d, Observations of the comet discovered by M. Gambart, July 19, 1832, and of Biela's comet. These latter observations have been given in the Monthly Notice for December 1832, in a communication from Professor Airy.

VI. Observations made at Saville Row. By Mr. Snow.

These observations comprise, 1st, Right ascensions of *Mars*, and of stars observed with *Mars*, in November and December 1832; 2d, Mean right ascension of 60 unknown stars for 1832; 3d, Observations of the egress of *Jupiter's* satellites on Dec. 26, 1832, made for the purpose of ascertaining if the phenomena could be observed with sufficient accuracy, agreeably to the recommendation of the superintendent of the *Nautical Almanac*.

" Dec. 26, 1832. Egress of first satellite at $23^h\ 12^m\ 40^s$ sidereal time. (Corrected for clock error.)

" At this point of time the satellite was just adhering to the edge of *Jupiter's* disc: but it had been distinguishable since $23^h\ 9^m$, or about 4^m sooner.

" The egress of the shadow of the first satellite could not be observed with any thing like accuracy with the instrument in question, for the shadow fell in the middle of the broad belt of *Jupiter:* clouds came over, too, at the time of the egress.

" The second satellite, which was set down as emerging at $1^h\ 16^m$ sidereal time, never emerged at all; at least, not before 3^h sidereal time, up to which time it was watched for continually without success.

" The third satellite disappeared finally at $2^h\ 19^m\ 0^s$, sid. time (corrected for clock error), pretty exactly. It was in contact with the limb of the planet 5 minutes before I lost sight of it. Telescope 42-inch refractor, power 120, which was found to succeed with *Jupiter* better than any other power; but the evening was so fine, that the telescope shewed *Jupiter* very well with a power of 300."

J. Moyes, Castle Street, Leicester Square.